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All in the Family: Partisan Disagreement and Electoral Mobilization in Intimate Networks - a Spillover Experiment*

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Abstract

We advance the debate about the impact of political disagreement in social networks on electoral participation by addressing issues of causal inference common in network studies, focusing on voters' most important context of interpersonal influence: the household. We leverage a randomly assigned spillover experiment conducted in the UK, combined with a detailed database of pre-treatment party preferences and public turnout records, to identify social influence within heterogeneous and homogeneous partisan households. Our results show that intra-household mobilization effects are larger as a result of campaign contact in heterogeneous than in homogeneous partisan households, and larger still when the partisan intensity of the message is exogenously increased, suggesting discussion rather than behavioral contagion as a mechanism. Our results qualify findings from influential observational studies, and suggest that within intimate social networks, negative correlations between political heterogeneity and electoral participation are unlikely to result from political disagreement.

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Introduction

In deciding whether to vote in an election, and which party to support, informal political discussions with family, friends and acquaintances play an important role. These discussions reinforce social norms, provide us with political information, allow us to understand the views and reasoning of others and to express –and maybe even persuade others of– our own views (Conover, Searing and Crewe 2002; Huckfeldt and Sprague 1987, 1991; Mutz 2002*b*, 2006; Sinclair 2012; Zuckerman, Dasović and Fitzgerald 2007). Ultimately they are said to result in increased political engagement and electoral mobilization (Cutts and Fieldhouse 2009; Huckfeldt and Sprague 1995; Klostad 2007, 2011, 2015; Lake and Huckfeldt 1998; McClurg 2003; Rolfe 2012; Sinclair 2012; Zuckerman 2005; Zuckerman et al. 2007).

Consequently, political scientists have long discussed the potential of campaigns to mobilize voters indirectly by encouraging political discussion within personal networks such as that of the family in the run-up to an election (McClurg 2003; Rolfe 2012). Glaser, for instance, argued that "the most influential canvassers are the rest of the people in the voter's family" and that "if party workers or civic organizations want the greatest return on the use of their scarce services, they should make contact with the most politicized member of a household [...] and motivate that member to bring all the other members to the polls" (1959: 570).

But, while studies show a strong relationship between political discussion within personal networks and political participation (Bond, Fariss, Jones, Kramer, Marlow, Settle and Fowler 2012; Klostad 2011, 2015; McClurg 2003; Rolfe 2012), there is an unresolved debate about the extent to which indirect mobilization is dependent on the degree of political agreement within the network (Bélanger and Eagles 2007; Fitzgerald and Curtis 2012; Huckfeldt, Johnson and Sprague 2002, 2004; Klostad 2011; Klostad, Sokhey and McClurg 2013; McClurg 2003, 2006; Mutz 2002*a*, 2006; Nir 2011; Pattie and Johnston 2009). In an influential series of observational studies Mutz (2002*a*; 2002*b*; 2006), while acknowledging the benefits of mixed political company for political tolerance, has expressed concerns about the correlation be-

tween network heterogeneity and electoral abstention. Her findings suggest that there might be a flipside to increasing tolerance, a demobilizing, "dark side of mixed political company" (Mutz 2006, 89). On the other hand, studies such as those conducted by Huckfeldt and colleagues (2002; 2004), Nir (2005) and McClurg (2006) dispute the demobilizing impact of disagreement within a network.

The literature on network heterogeneity and political participation has faced difficulty in making strong causal inferences (Fowler, Heaney, Nickerson, Padgett and Sinclair 2011; Klofstad 2007, 2011; Mutz 2006). Findings from studies relying on randomized campaign experiments, which allow for stronger causal inferences, support the notion of indirect mobilization through small, personal networks by showing that conversations with campaign volunteers not only mobilize the contacted individual, but also their household members – a phenomenon that is called "spillover" (Nickerson 2008; Sinclair 2012; Sinclair, McConnell and Green 2012). Yet, spillover experiments have not addressed the potential consequences of political (dis)agreement for intra-household mobilization. In addition, these experiments have exclusively relied on non-partisan mobilization messages, notwithstanding the partisan nature of most campaigns. This while the partisan intensity of mobilization messages likely impacts mobilization among members of a household differently depending on whether they are politically like-minded or non-like-minded.

In this paper we integrate the literature on political homo-and heterogeneity in social networks and the experimental literature on indirect campaign mobilization. We focus on what is arguably the most important context of interpersonal influence for voters: the household (Cutts and Fieldhouse 2009; Fieldhouse and Cutts 2012; Nickerson 2008; Sinclair et al. 2012; Stoker and Jennings 1995; Zuckerman et al. 2007). While there is a high congruence between household members' party and policy preferences (Stoker and Jennings 2005), still no more than an estimated 40-60 percent of two-voter households can be described as politically homogeneous in partisan terms (Bélanger and Eagles 2007; Johnston, Jones, Propper, Sarker, Burgess and Bolster 2005; Zuckerman, Fitzgerald and Dasović 2005). We ask to what

extent intra-household mobilization during an election campaign is conditioned by both the degree of heterogeneity of party preferences within the household, and the partisan intensity of a campaign message. We focus on two-voter households, the majority of which is likely to consist of family members, in particular couples. Following Huckfeldt et al. (2004) we conceptualize disagreement as partisan disagreement (for a discussion, see Klofstad et al. 2013). Instead of relying on only one network member’s report of the extent of partisan disagreement in their network, though, we use the self-reported party preference of each household member, thus reducing the systematic biases in respondents’ perceptions (Klofstad et al. 2013; Osborn and Mendez 2011).

Our research design combines data from a previously conducted randomized campaign experiment in the United Kingdom (UK), which includes information on the party preferences of household members, with validated turnout data from the official voter register on both assigned subjects and those household members excluded from the original experiment. We conduct an implicit mediation analysis (Gerber and Green 2012) by utilizing campaign messages of different partisan intensity that manipulate the potential for partisan disagreement within heterogeneous and homogeneous households, and assess their impact on turnout. Our results show that intra-household mobilization effects are larger as a result of campaign contact in households that consist of individuals who hold competing party preferences, and larger still when the partisan intensity of the message is increased. Thus, we find no evidence in support of a demobilizing impact of disagreement. In contrast, partisan disagreement within the confined space of the household might be beneficial for encouraging political debate and ultimately participation.

Social Influence in Partisan Households

Early voting studies established the importance of political discussion within personal networks for voting behavior (Berelson, Lazarsfeld and McPhee 1954; Campbell, Converse,

Miller and Stokes 1960; Downs 1957; Lazarsfeld, Berelson and Gaudet 1948). Ever since these early studies there has been a debate about the importance of the nature of the network (Lazarsfeld et al. 1948). Currently, an important aspect of this debate concerns the question whether discussion is more likely in networks that consist of politically like-minded or non-like-minded individuals, and how this so-called network homo- and heterogeneity (Mutz 2006) ultimately impacts on political participation.

It is important to note that much of this current debate does not explicitly address households (see for notable exceptions: Bélanger and Eagles 2007; Bello and Rolfe 2014). Yet, family or household members are regularly named as the most important or frequent discussion partners (Mutz 2006, 126; Sinclair 2012, 27). Couples have been shown to influence each other, to become more similar in their party preferences (Stoker and Jennings 2005; Verba, Schlozman and Burns 2005; Zuckerman et al. 2007), and to exhibit similar levels of turnout (Glaser 1959; Straits 1990; Verba et al. 2005).

Mutz (2002*a*; 2006) shows that individuals in heterogeneous networks are less likely to participate in politics, and argues that this is due to the experienced cross-pressures resulting from the diverging political views within the network. Specifically, Mutz (2006) suggests that there might be two, inter-connected, psychological processes at work. First, membership in a politically heterogeneous network may lead to internal ambivalence regarding one's own political position. This ambivalence, in turn, might lead to negative participation decisions. Second and most importantly, because individuals try to maintain social harmony within their personal networks by pleasing all members they avoid discussion of divisive political issues, which in turn discourages participation. An additional potential mechanism, discussed by Huckfeldt et al. (2004, 7) builds on Downs's (1957) argument that individuals seek out discussion partners within their personal networks that are well-informed and have similar political viewpoints as their own as a means to obtain low-cost information. Consequently, this mechanism also suggests that political disagreement within personal discussion networks is avoided, albeit inadvertently.

When it comes to households, it is certainly plausible that these processes work similarly. Maintaining social harmony might be particularly important with those with whom we live under one roof, and unlikely to be jeopardized over political issues that are of limited interest to the majority of individuals (Mutz 2006, 106). In addition, we might avoid political discussion with household members if we do not perceive them as politically agreeable, and therefore useful, sources of information. Among the few observational studies that have looked explicitly at households, there is some empirical support for the notion that individuals in homogeneous – all-Democratic or all-Republican – households are indeed more likely to vote than those in households in which members differ in their party registration (Bélanger and Eagles 2007).

On the other hand, exactly because the household tends to constitute our most intimate personal network, it can arguably endure a substantial amount of disagreement when it comes to issues that do not directly affect personal relationships between family members (Morey, Eveland and Hutchens 2012). In fact, political discussions might be difficult to avoid with people with whom one interacts so frequently, often occurring as by-products of sharing the same household, and certainly not as easy to shun as unwelcome TV channels or online news sites. Like with sports, we might enjoy discussing and watching a game with household members even if we support different teams.

The expectation that individuals who live in politically heterogeneous households are able to disagree without being disagreeable is also based on the observation that individuals in heterogeneous networks are more tolerant to other political viewpoints than individuals in homogeneous discussion networks (Mutz 2002*b*; 2006). Indeed, Mutz’s (2002*b*) findings that political discussion and political tolerance is higher in heterogeneous networks appears to stand in contrast to a theory that would expect household members to disengage from politics in order to avoid conflict and confrontation. Bello and Rolfe (2014) show that Mutz’s (2002*b*) finding also generalizes to the UK, and specifically to the household. Moreover, Klostad (2011) shows that social intimacy (trust) strengthens the relationship between discussion

and civic participation.

Not only might political discussion be as frequent in heterogeneous as in homogeneous households, there are reasons to suspect it might be more frequent. Bello and Rolfe (2014) show that not only are "[s]pouses and family members [...] more likely to be retained as political discussants than other friends, even close friends", "respondents are more likely to continue discussing politics with spouses who hold different political views when compared to spouses who hold the same view" (Bello and Rolfe 2014, 141). This might be because, given the already high level of homophily in terms of socioeconomic characteristics and the living environment, discussing with a household member who thinks differently about politics provides a more effective means of gaining additional information about an upcoming election than talking to a co-partisan. Alternatively, within the context of an election campaign, political disagreement might create a situation of friendly competition between household members that leads to higher levels of political engagement, and ultimately electoral participation. Politically interested individuals – often partisans – are more likely to discuss politics, even though this increases the probability of political disagreement, because they simply enjoy political discussion (Huckfeldt and Mendez 2008). Competition between household members might therefore either increase electoral participation indirectly through fostering political discussion, or directly increase turnout if household members decide to "counter-mobilize", voting for the opposing candidate in order to "cancel out" the vote of their household member (Straits 1990, 64-65; Wolff 2002, 33).

The Challenge of Making Causal Inferences

One of the major challenges of research on the impact of network diversity on political participation is separating self-selection into politically like-minded personal discussion networks, and contextual influences, from social influence within networks (Fowler et al. 2011; Klostad 2007, 2011; Manski 1993). Self-selection can occur when characteristics of individuals impact

both the level of heterogeneity in their discussion network and their level of political participation (Mutz 2006, 115; Bélanger and Eagles 2007, 850). For instance, individuals who are more politically interested or have stronger partisan preferences might be more likely to participate as well as to surround themselves with like-minded discussion partners (Mutz 2006, 115). Contextual influences occur when the external environment has a similar, exogenous, impact on all members of a network – for example a political campaign could change network members’ political views and participation rates in the same direction, without individuals influencing each other (Manski 1993; Fowler et al. 2011, 450).

A few studies have exploited the benefits of panel data (Bello and Rolfe 2014) or statistical techniques such as propensity score matching (Klofstad et al. 2013) for causal inference. While certainly an improvement on earlier correlational work, such studies do not fully resolve the issue of separating self-selection and contextual influences from social influence as they do not overcome the issue of unobserved individual or contextual-level confounders (Arceneaux, Gerber and Green 2006; Wooldridge 2010). For example, when it comes to campaign-induced discussion, homogeneous partisan households might have a higher probability of being contacted by election campaigns than heterogeneous partisan households because the former provide the opportunity to mobilize several potential party supporters with one contact.

A handful of scholars has conducted studies in which households are randomly allocated to be contacted by non-partisan campaigns, ensuring that households exposed to such an external shock are in expectation the same as households that are not (Nickerson 2008; Sinclair 2012; Sinclair et al. 2012). Differences in the outcome of interest should consequently be due to social influence within the household, and not to pre-existing compositional and contextual differences between households. These randomized field experiments have shown that turnout levels among household members are significantly higher because their family members were contacted by election campaigns, confirming that contagion of campaign

messages indeed occurs (*idem*).¹

Contribution and Limitations

While randomized spillover experiments have contributed much to our understanding of the household as the most important location for intra-personal mobilization, the household itself has remained a "black box" for experimentalists (Fowler et al. 2011; Nickerson 2008; Sinclair et al. 2012). Field experiments have neither been able to distinguish between households according to the composition of their members' pre-existing partisan preferences, nor have they been able to ascertain the impact of (discussions about) explicitly partisan campaigns on electoral mobilization, as all spillover experiments have been based on non-partisan campaign interventions.

The contribution of this study lies in addressing these omissions. By utilizing a spillover experimental design we are able to separate selection effects and pre-existing contextual differences from social influence in assessing to what extent intra-household mobilization occurs as a result of an election campaign. Subsequently, we test whether such mobilization is conditioned by the degree of heterogeneity of party preferences within the household. We perform an implicit mediation analysis (Gerber and Green 2012, 333-336) by randomly assigning one of two campaign messages of varying partisan intensity, hence further manipulating the potential for discussion between household members. Our argument is that overtly partisan messages exacerbate partisan sentiment compared to more neutral messages, making pre-existing partisan disagreement within the household more salient. In part this might be because overtly partisan messages function as a social identity cue, rallying support for the team and pitting party supporters against each other (Green, Palmquist and Schickler 2002; Rogers, Fox and Gerber 2014). An overview of our study variables and hypothesized

¹Based on a natural experiment that exploits random assignment to college dorm rooms, Klostad (2007; 2011; 2015) also provides evidence of social influence by showing that being randomly exposed to political discussion leads to increased civic and political participation in and after college.

relationships is shown in Figure 1.

[Figure 1 about here]

While our study thus allows for a unique contribution to both the literatures on social network composition and on voter mobilization, it is important to point out its limitations. First, although our study design allows us to identify the causal impact of campaign contact on intra-household mobilization within both heterogeneous and homogeneous partisan households, we prime, but do not randomly assign partisanship within households. Consequently, we cannot exclude the possibility that compositional partisan differences might be confounded by a number of (un)observable background attributes. Individuals in heterogeneous partisan households might, for instance, be *ex ante* less likely to participate in politics than individuals who live in homogeneous partisan households. While we therefore cannot claim that the *conditional effects* that we uncover are causal in nature, we utilize information about turnout behavior in multiple previous elections to estimate the effects net of *ex ante* turnout differences between heterogeneous and homogeneous households.

Second, we share the limitation present in the experimental and observational literature that we cannot conclusively show that contagion within households is a result of actual discussion (Fowler et al. 2011, 463; Nickerson 2008). As Bullock, Green and Ha (2010) have demonstrated, it is almost impossible to identify a causal mediator, even if a direct measure of discussion was available in our dataset. Hence, we must acknowledge the possibility of alternative explanations for any indirect mobilization effects identified in our experiment that may occur in the (near) absence of discussion. Household members might simply have listened in on the experimental subject’s telephone conversation with the party volunteer and adjusted their behavior accordingly. Alternatively, they might have felt social pressure after observing the experimental subject going out to vote, or they might have considered the lower costs of voting by going to the polls together (Nickerson 2008, 55). Most of these alternative mechanisms, however, differ in their observable implications. In the latter two

scenarios in particular, there would be little reason to expect different levels of indirect mobilization depending on whether the household is homo- or heterogeneous, or depending on the partisan intensity of the campaign message.

A third limitation concerns the question of generalizability (Fowler et al. 2011, 466). The mobilization messages used in our study were communicated by a local Labour Party. Work by Jost and colleagues suggests that individuals who hold conservative ideologies are more likely to avoid situations that arouse cognitive dissonance and less likely to be open to new experiences and arguments than liberals (Carney, Jost, Gosling and Potter 2008; Garrett 2009; Jost, Glaser, Kruglanski and Sulloway 2003; Nam, Jost and Bavel 2013). Consistent with this idea, Mutz (2006, 33) shows that conservatives are less likely to be embedded in cross-cutting political discussion networks than liberals. Consequently, we cannot exclude the possibility that conservatives might be less responsive to a Labour campaign message than Labour supporters to a Conservative campaign message. Moreover, within the household conservatives might be less likely to share opinion-challenging messages with Labour supporters than vice versa. Hence, we would expect indirect mobilization effects to be more pronounced if contact was made by a Conservative campaign and mediated by a subject who has liberal leanings than the other way around. Our focus on campaign contact also means that our findings might not generalize to other settings in which political discussion is likely to occur within the household, such as when watching news programs together.

Finally, although households are important locations of social interaction, they are also unique because personal relationships between household members are usually stronger and may therefore withstand political disagreement more easily than ties that exist between colleagues or acquaintances. This study therefore seeks to qualify, but not directly contradict the empirical findings of previous work such as Mutz’s (2006), whose data on cross-cutting exposure includes weak ties such as those formed at the workplace or in places of worship.

Notwithstanding these limitations, the household is arguably the most important personal discussion network and hugely influential in our turnout decisions. By examining the

conditions under which campaign-induced partisan disagreement between household members translates into turnout, we aim to shed some light on intra-household mobilization, and to more closely integrate, and make valuable contributions to, the literatures on social network composition and campaign mobilization.

Research Design

We rely on a previously conducted randomized campaign experiment to identify mobilization effects between household members in two-voter households, and to gauge the extent to which they are conditioned by both the degree of heterogeneity of party preferences within the household and the partisan intensity of the campaign messages. We conducted this campaign experiment in several electoral wards in the British city of Birmingham in the context of the West Midlands Police and Crime Commissioner (PCC) election, which was held on November 15th 2012.² This campaign experiment combined two design innovations that make it especially well-suited for our current study.

First, the experiment was conducted in co-operation with a local Labour Party. Existing field experimental studies of interpersonal influence within households have so far exclusively relied on identifying spillover effects of non-partisan Get-Out-The-Vote (GOTV) interventions. When trying to assess the impact of an election campaign on indirect mobilization within households, working from within a party's campaign mimics reality more closely given that most mobilization efforts tend to be partisan in nature. As we discuss below, phone bank volunteers were instructed to use two different messages: one of low and one of high partisan intensity. This difference allows us to identify how varying the potential for partisan disagreement affects intra-household mobilization.

Second, we had access to detailed information on the pre-treatment party preferences of both household members. This information was included in the Labour Party's extensive

²For a detailed description of the data, see the Replication Data Codebook, available at: <http://dx.doi.org/10.7910/DVN/ZFLG25>.

targeting database, and is based on prior canvassing efforts conducted by party volunteers in the constituency.³ The party aims to contact every voter living in the parliamentary constituency to find out which party he or she supports. It is common practice for constituents to volunteer their voting intentions to party canvassers in the UK. The database covered around 38% of registered voters. Although the accuracy of the pre-treatment party support measure obtained through local Labour Party canvassing is difficult to assess, a comparison to City Council election results suggests that neither Labour supporters, nor supporters of rival parties are overrepresented in the data (Supporting Information, Table A7).⁴ In addition to party preference, the database also provided information on gender, year of birth, whether individuals were registered as postal voters, the electoral ward in which they reside, and individuals' validated turnout histories (as available from public records). This extensive set of covariates allows us to check whether our findings are robust to covariate adjustment.

Experimental Assignment

To fulfill the non-interference assumption (SUTVA), we randomly selected one individual per household included in the electoral database to be part of the original experiment. The household members of these experimental subjects were not assigned to receive any treatment. For random assignment, a total of 13,065 households with landline numbers (of which 5,190 were two-voter households) were first stratified into three blocks based on the lat-

³We were provided with access to a fully anonymized version of the database that included all registered voters with available landline or mobile phone numbers, gained informed consent from the Constituency Labour Party to conduct this experiment, and approval from the internal review boards of both authors' universities.

⁴For the purpose of a separate field experiment conducted elsewhere (Foos 2015, chapter 3), one of us validated the party support measure used in this study with a comparable measure obtained from independently conducted phone interviews on a sample of rival party supporters. Interviews with subjects classified as rival party supporters showed that 72% of respondents in the untreated control group were correctly classified. Only 9% classified as rival party supporters by canvassers told interviewers that they supported the Labour Party or a Labour candidate instead.

est recorded party preference of the experimental subject: ‘Labour’ supporter, ‘rival party’ supporter (i.e. Conservative, Liberal Democrat, Green, BNP, Respect, UK Independence Party, or those who otherwise indicated to explicitly oppose Labour), and those who were ‘unattached’ to a party or whose latest recorded party preference was unknown (i.e. ‘won’t say’, ‘don’t know’, ‘non-voter’ or ‘missing’). The choice of these three groups was motivated by the design of the original experiment, which aimed to distinguish Labour from rival party supporters. Homogeneous partisan households are accordingly defined as households that either include two Labour voters or two rival party supporters; heterogeneous partisan households as consisting of one Labour and one rival party supporter; and unattached households as consisting of at least one unattached voter.⁵ Next, for two-voter households, experimental subjects (and their household members) were, within each partisan group, randomly assigned to either one of two treatment groups or to the control group. The control group did not receive any form of contact from the campaign. An overview of the random assignment within partisan blocks for two-voter households is shown in Figure 2.

[Figure 2 about here]

We followed standard randomization inference procedure (Gerber and Green 2012) to check if any covariate imbalances in our sample of two-voter households were larger than expected given random sampling variability. The resulting p-value of .50 indicates that we cannot reject the sharp null hypothesis that the covariates taken together are not systematically related to treatment assignment of the household member. Detailed balance

⁵Among rival party supporters in our sample, 49% indicated support for the Conservative Party, less than 5% explicitly mentioned the name of another party or of an independent candidate, and 47% stated to be ‘against’ Labour. The latter are most likely Conservative supporters or, alternatively, supporters of another right-wing party. This means that we cannot exclude the possibility that some homogenous partisan households consisting of two rival party supporters are actually heterogeneous. In the analysis section we demonstrate that our findings remain robust when distinguishing homogenous Labour households from households consisting of two rival party supporters.

tables and figures can be found in the Supporting Information (Table A1, and Figure A1). Furthermore, as we would expect given that turnout data is collected from the public register, missing outcome data for any household member is neither systematically related to treatment assignment alone, nor is it a function of treatment assignment, pre-treatment covariates and the interaction between treatment assignment and covariates (Figure A2 in the Supporting Information).

In order to estimate the within household spillover effects resulting from the two treatments, we compare turnout among unassigned household members living with subjects who were assigned to one of the two treatment conditions to turnout among unassigned household members living with subjects assigned to the control condition. In doing so, our spillover model relies on the common assumption that effects spill over within households, but not between neighboring households. This assumption has been tested and confirmed in previous field experiments (Sinclair et al. 2012) and we have no reason to believe that it was violated in the context of our experiment.

Treatments

Experimental subjects allocated to the two treatment groups were called by telephone by Labour Party volunteers in the week leading up to the election (November 10th – 15th), and were encouraged to vote in the PCC election on November 15th 2012. Phone bank volunteers were entirely blind to subjects’ pre-treatment party preferences, and were instructed to ask to speak to the person in the experimental group before delivering the messages. In case the person was unavailable, volunteers were instructed to politely finish the conversation without revealing the intent of the call.

In formulating the message scripts, we worked closely with the local Labour Party in order to ensure the messages paralleled normal campaign efforts. Both messages provided practical information to subjects on the election date and their local polling station, and encouraged them to vote for the same candidate, providing information about the candidate’s

background and his most important policy stance (opposition to cuts in police numbers). However, the content of the scripts varied drastically across the two treatment groups. The ‘high partisan intensity’ treatment was formulated in a strongly partisan tone, explicitly mentioning the Labour Party and policies multiple times, while taking an antagonistic stance toward the main rival party, the Conservative Party, by pointing out the Labour candidate’s opposition to the "Tory cuts" in police numbers. In contrast, the ‘low partisan intensity’ treatment message avoided all statements about party competition, and mentioned neither the candidate’s party affiliation, nor the rival party. Both message scripts are displayed in Figures A3 and A4 in the Supporting Information.

Campaign volunteers were asked to fill in a form on whether contact with the targeted individual was made, any reasons for why contact had failed (i.e. answering machine, no answer, hang-up, etc.), and the number of call-backs made. After the election the local Labour Party provided us with an updated version of the (anonymized) database, which included validated turnout data for the PCC election from the marked electoral register for both the experimental subjects and their household members. Before the analysis, all two-voter households with missing outcome data were removed from the sample.

We took great care in the parallel administration of both treatments. The total contact rate, defined as the percentage of individuals in the respective treatment groups who answered the phone when called by volunteers, among two-voter households, is 45% for both treatment groups (Table 1). Thus, we failed to administer the treatment to 55% of those assigned to treatment. This rate of noncompliance is similar to the rate recorded in other GOTV studies in the UK (John and Brannan 2008).

Analysis and Results

We start by examining to what extent low and high partisan intensity phone messages mobilized experimental subjects and their household members. Table 1 shows turnout rates

for experimental subjects and their household members assigned to the control group and to the two treatment groups. These rates are shown by experimental block, meaning conditional on the pre-treatment party preference of the assigned subject.

The final column shows that turnout rates among subjects assigned to the control group and their unassigned household members are almost identical at around 19-20 percent. We would expect this given the stratified random sampling procedure used to allocate one subject per household to the experimental sample. Among subjects assigned to receive either a low or high partisan intensity phone call turnout reaches 22-23 percent. Turnout among the household members of these subjects is slightly higher at 24 percent. Using randomization-inference (Aronow and Samii 2012; Gerber and Green 2012) to estimate p-values and confidence intervals, we find that, for all partisan groups combined, both the direct and the indirect Intent-to-Treat (ITT) effects are statistically different from the respective control groups with $p < 0.05$. The results also suggest that both the low and high partisan intensity campaign messages resulted in intra-household mobilization. As we would expect, Table A2 in the Supporting Information shows that the estimates are robust to covariate-adjustment.

[Table 1 about here]

We next test whether the rates of intra-household mobilization differ significantly between homogeneous and heterogeneous partisan households. Figure 3a displays the changes in the predicted probabilities of turnout for unassigned subjects as a function of the treatment assignment of their household member, conditional on the partisan composition of the household. The predicted probabilities are based on a logistic regression of turnout on treatment assignment of the household member, the partisan composition of the household, the interaction between treatment assignment and household partisan composition, pre-treatment covariates and the interaction between pre-treatment covariates and treatment assignment (see equation 14 and Table A3, Model III in the Supporting Information).

[Figure 3 about here]

The key result is that intra-household mobilization effects are stronger, not weaker, in households where individuals support different parties. While indirect mobilization effects are around 3.5 percentage-points in homogeneous partisan households and around 7 percentage-points in households with at least one unattached voter, spillover effects in heterogeneous partisan households peak at around 15 percentage-points. Although we are dealing with a treatment-by-covariate interaction, this relationship remains unchanged regardless of whether we adjust for pre-treatment individual and compositional differences in age, gender, place of residence, registration type (postal voter or not), and turnout history (see models I - III in Table A4 in the Supporting Information). Moreover, Figure 3b shows that our results are robust to further breaking down the homogeneous partisan household category into homogeneous Labour households and homogeneous rival party households, and the unattached category into households that include only one unattached subject and households in which both members are unattached.⁶

What do these results tell us? We find no evidence to support the hypothesis that individuals are more likely to mobilize a co-partisan than a household member who supports another party as a result of being contacted by the Labour campaign. Although we find that campaign messages spill over in homogenous households consisting of two Labour Party supporters or which include an unattached voter, mobilization effects in these households are no greater than in heterogeneous partisan households. In contrast, we find greater spillover effects when partisans support different parties.

Interaction Dynamics Between Household Members

The finding that campaign spillover effects appear more pronounced in heterogeneous than in homogeneous partisan households is difficult to reconcile with a theoretical conception of household members as conflict avoiders. In what follows we try to further disentangle the

⁶The marginal changes in predicted probabilities in Figure 3b are based on estimates from model III, Table A4 in the Supporting Information.

partisan mobilization dynamics within the household by examining whether spillover is more or less likely depending on the partisan preferences of both household members.

Figure 4 displays the marginal changes in predicted probabilities resulting from a logistic regression of turnout on assignment to either of the two treatments, the partisanship of the experimental subject, the partisanship of her household member and the two- and three-way interactions between the treatment and the party preferences of both household members. The model also includes pre-treatment covariates, and interactions between covariates and the treatment (for estimates see models I-III, Table A5 in the Supporting Information).

[Figure 4 about here]

Figure 4a displays the indirect mobilization effects if the assigned household member supports a rival party, 4b if the assigned household member supports Labour, and 4c if the assigned subject is unattached to any party. The rival, Labour and unattached labels listed on the X-axis refer to the party preference of the unassigned household member.

Figure 4a shows that rival party supporters mobilize Labour supporters if they are contacted by a Labour campaign. Figure 4b also shows that Labour party supporters mobilize rival party supporters as a consequence of Labour campaign contact. While the effects for heterogeneous households are noisy due to small sample sizes, they are substantially large, and seem even more pronounced than indirect mobilization effects between two Labour supporters (although the difference between the two effects is not statistically significant). These results thus suggest that party supporters discuss campaign messages with their household members even if a party they oppose initiates contact.

Implicit Mediation Analysis: Does Increasing Partisan Intensity Affect Spillover?

The previous results demonstrate that within household mobilization plays an important role in partisan election campaigns and that, in contrast to what a conflict-avoidance mech-

anism would suggest, heterogeneous partisan households are conducive to indirect campaign mobilization. We implicitly test the effects of raising the level of partisan discussion within the household by exploiting the design feature that the partisan intensity of the campaign message was randomly assigned. Messages with stronger partisan language should exacerbate partisan sentiment compared to more neutral messages, making pre-existing partisan disagreement within the household more salient, and resulting in a higher probability of partisan discussions in the household.

Table 2 and Figure 5 show the logistic regression estimates and the corresponding predicted Conditional Complier Average Causal Effects (CACE) comparing outcomes directly for subjects whose household members answered the high intensity phone call to subjects whose household members answered the low partisan intensity phone call. Since subjects included in the original experiment did not know before answering the phone whether they were about to receive the high or the low partisan intensity call, and compliance is defined as living in a household where the assigned subject would answer the phone when called, the share of compliers in the high and the low partisan intensity call groups should, in expectation, be identical (Gerber, Green, Kaplan and Kern 2010, 302-305). Following Gerber et al. (2010) this "perfect blindness" assumption can be assessed empirically. If, as in our case, compliance at 45.4% and 44.8% does not vary as a function of treatment assignment, we can compare turnout rates for household members of compliers directly to each other (for the formal assumption, and the corresponding CACE spillover estimator see Equations 23 and 24 in the Supporting Information).

[Table 2 about here]

Across partisan blocks, the results in columns 1 and 2 of Table 2 confirm that receiving a high intensity partisan message did not result in significantly lower levels of intra-household mobilization than receiving a message without partisan content. The relative effectiveness of the two messages again seems to differ according to the partisan make-up of the household.

Columns 3, 4 and 5 display the results of models that include interactions between the partisan composition of the household and the partisan content of the message. While column 4 shows the estimates from a model unadjusted for compositional differences between homogeneous and heterogeneous households, column 5 shows the estimates for a model that includes interactions between pre-treatment covariates and the treatment. The statistically significant interaction terms indicate that increasing the potential for partisan disagreement had a significantly more positive effect on turnout for household members living in heterogeneous partisan households than for household members living in homogeneous and unattached partisan households.

Figure 5 displays the marginal changes in predicted probabilities of turnout for unassigned subjects based on model V in Table 2. Figure 5b shows a robustness check which compares heterogeneous households separately to homogeneous Labour households and households in which both members support rival parties (based on Model V in Table A6 in the Supporting Information). If partisan discussion is a plausible mediator linking campaign contact to intra-household mobilization, raising the partisan intensity of the message should increase, not decrease indirect mobilization effects in heterogeneous households.

[Figure 5 about here]

In line with this expectation, we find that indirect mobilization effects in heterogeneous households are 35 percentage-points larger among individuals living with someone who received a call priming her partisan identity than among subjects whose household members received a call of low partisan intensity. This substantial, but noisy CACE estimate might be a function of the low baseline turnout rate, and the relatively small subsample size when comparing household members of compliers directly to each other. It is hence likely that we are dealing with an upper bound on the true effect size. Although we therefore caution against overinterpreting the size of this effect, we clearly do not find any evidence that would support the notion that priming household members' party preferences would *decrease*

indirect mobilization between subjects who are predisposed to disagree on which party to support.

Discussion and Conclusion

An increasing number of political scientists view turnout as a social activity, and few doubt that family, friends and neighbors influence decisions about whether or not to participate politically and who to support (Huckfeldt and Sprague 1995; Klofstad 2011; Mutz 2006; Rolfe 2012; Sinclair 2012; Zuckerman 2005; Zuckerman et al. 2007). The debate within the literature now centers on whether political disagreement between network members is harmful or beneficial for political participation. In this paper we contribute to this debate by focusing on a specific, but very common, instance of political disagreement that occurs among household members in the context of a partisan election campaign and which affects the decision to vote. Our study is unique because it combines the exogenous assignment of partisan telephone calls with an unusually rich dataset, made available by a local branch of the UK Labour Party, which includes the party preferences and validated turnout of all household members. Our results demonstrate that within household mobilization effects are considerably larger as a result of campaign contact in households that consist of members who disagree in their party preference than in households in which members share a similar party preference.

These results provide an important qualification regarding the role of disagreement in discouraging electoral participation in the context of the household. Rather than supporting a demobilizing impact of partisan disagreement, our results offer new, experimental evidence for the participatory benefits of disagreement that advance on observational work carried out by Huckfeldt and colleagues (2002; 2004), Nir (2005) and McClurg (2006). Importantly, our study is the first to causally identify the indirect effects of partisan messages on turnout in heterogeneous and homogeneous personal networks. We recognize that our findings may

be limited to the household or other equally intimate settings, and may not apply to social networks in which ties are weaker, and political discussion more easily avoidable. We have suggested that because households tend to be high-trust environments, the risk of potential fallout from political discussion is likely lower than in environments such as the workplace. If social costs are lower, individuals can more easily enjoy the entertainment value and political information that discussions about partisan politics provide. As such, our findings qualify, but do not directly contradict, Mutz’s (2006) earlier work on turnout in heterogeneous networks that consist of both strong and weak ties.

How confident are we that this intra-household mobilization is a result of discussion rather than some other form of social influence? Social influence can occur in a number of imaginable ways. For instance, observing a household member voting might function as a simple reminder to vote or it may provide social pressure to comply with a perceived norm of voting. By showing evidence that campaign messages of a highly partisan nature increase indirect mobilization in heterogeneous partisan households to a greater extent than campaign messages of a lower partisan intensity, our findings are suggestive of household members talking about the message, rather than solely acting as a consequence of observing their household member’s act of voting. Our assertion that members in two-voter households do not refrain from partisan discussions in order to avoid conflict is also in line with research that shows how couples who live together become increasingly similar over time in their policy and party preferences (Stoker and Jennings 2005; Zuckerman et al. 2005). In order to adopt each other’s preferences surely couples must be talking.

In addition to the literature on disagreement in social networks, this paper also makes an important contribution to the literature on partisan election campaigns. While the existence of an indirect mobilization effect within households has been demonstrated in non-partisan GOTV experiments (Nickerson 2008; Sinclair et al. 2012), we show that this effect is also present in partisan campaigns. As Nickerson and colleagues (2006) caution, partisan and non-partisan campaigns differ in multiple ways, and intra-household partisan dynamics are

widely expected to influence the effectiveness of indirect campaign mobilization (Huckfeldt et al. 2004). It remains to be seen to what extent indirect mobilization effects between partisans are replicated, or even amplified, when conservative parties initiate contact with supporters of more liberal, rival parties, as liberals are said to be more open to opinion-challenging information than conservatives (Jost et al. 2003; Nam et al. 2013).

The relatively large spillover effects reported in this paper (cf. Nickerson 2008; Sinclair et al. 2012) might partly be due to the low-information, low-saliency nature of the election. Alternatively, it might be a function of the research design. In a recent paper, Mann and Sinclair (2014) argue that spillover effects might differ depending on whether the low- or the high-propensity voter in a household is targeted. As a function of our research design, the lower propensity voter had an equal probability of being assigned to contact as the higher propensity voter. In contrast, in Nickerson’s (2008) experimental design the higher propensity voter might have been more likely to self-select into opening the door to the canvasser. Even if our results are on the high end of the scale, they nonetheless strongly suggest that partisan election campaigns can have substantial indirect mobilization effects that should be taken into account when assessing the cost-effectiveness of different voter mobilization methods.

Our results thus raise intriguing strategic questions for partisan campaigns. Partisans appear to be more responsive to campaign messages from rival parties if these messages are mediated by household members than if they are transmitted directly by party canvassers. Our results suggest that campaigns can indirectly affect the turnout behavior of a rival party’s supporters via sympathetic household members. Future research should hence investigate whether a campaign strategy that targets heterogeneous partisan households is prone to be strategically ineffective by mobilizing votes for opposing parties, or if such a strategy might be beneficial to parties if contacted supporters persuade their household members to switch their vote.

Are these results specific to the UK context? We sincerely doubt so. Compared to the

US, the partisan context in the UK is less polarized, and fewer individuals hold strong party preferences (Ford 2014; Pattie and Johnston 2010). As such, one might have expected no difference in intra-household mobilization between households in which members support the same party and those in which they do not. Our finding that spillover effects are larger as a result of campaign contact in heterogeneous than in homogeneous partisan households, and larger still when the partisan intensity of the message is increased, would lead us to expect an even more pronounced impact of partisan disagreement within households on electoral participation in the US context.

Political discussion within personal networks is widely believed to go hand in hand with increasing levels of political knowledge and tolerance of competing political viewpoints, ultimately benefiting the functioning of democracies (Conover et al. 2002; Mutz 2002*b*, 2006). However, scholars have doubted that these civic characteristics of politically heterogeneous networks also facilitate political participation (McClurg 2003; Mutz 2002*a*, 2006). Our paper shows that political disengagement is not necessarily the flip side of the political diversity and tolerance fostered by sharing the same household with individuals who hold different political views. A better understanding of how partisan political disagreement within the family can motivate voter participation provides valuable insights into the general conditions under which political discussion in personal networks occurs and under which it generates civic benefits.

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Table 1: Turnout and Contact Percentages in Treatment and Control Groups

	By Partisanship of Assigned Subject			
	Labour	Rival Party	Unattached	Combined
	Assigned Subjects			
Turnout Control	25.0	24.1	11.6	19.2
Turnout Low Partisan Intensity	27.2	25.4	17.3	22.6
Turnout High Partisan Intensity	29.6	26.2	14.7	22.4
ITT Low Partisan vs Control	2.2	1.4	5.8**	3.4*
	[-3.7, 8.3]	[-4.0, 6.7]	[2.1, 9.4]	[0.6, 6.2]
ITT High Partisan vs Control	4.6	2.1	3.1	3.2*
	[-1.5, 10.7]	[-3.1, 7.3]	[-0.4, 6.9]	[0.5, 5.9]
	Unassigned Household Members			
Turnout Control	24.7	24.7	12.7	19.8
Turnout Low Partisan Intensity	28.3	27.7	19.3	24.4
Turnout High Partisan Intensity	28.2	28.7	16.6	23.6
ITT Low Partisan vs Control	3.5	3.0	6.6***	4.6**
	[-2.4, 9.6]	[-2.5, 8.3]	[2.7, 10.5]	[1.8, 7.5]
ITT High Partisan vs Control	3.4	3.9	3.9*	3.8**
	[-2.5, 9.4]	[-1.3, 9.2]	[0.2, 7.9]	[1.0, 6.6]
Contact Rates Low Partisan Intensity	53.6	46.0	39.7	45.4
Contact Rates High Partisan Intensity	47.3	50.4	38.8	44.8
N	1273	1635	2022	4930

Note: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Two-tailed tests based on randomization-inference. Table excludes subjects in households with missing turnout data. Combined column includes inverse probability weights accounting for different probabilities of assignment to experimental conditions between blocks.

Table 2: Logistic Regression Results: CACE of high vs low partisan intensity call on turnout of unassigned subjects, conditional on household party preferences

	I	II	III	IV	V
Phone Call	-.109 (.142)	-.154 (.165)	1.337 (.687)	1.407 (.785)	1.824 (1.364)
Heterogeneous Partisan	Reference Group				
Homogeneous Partisan	-.202 (.325)	-.687 (.383)	.688 (.581)	.256 (.648)	.406 (.656)
Unattached	-.571 (.364)	-.648 (.431)	.399 (.604)	.363 (.680)	.534 (.690)
Heterogeneous x Call	Reference Group				
Homogeneous x Call			-1.437* (.716)	-1.578 (.818)	-1.823* (.835)
Unattached x Call			-1.606* (.719)	-1.713* (.824)	-1.976* (.841)
Covariates	No	Yes	No	Yes	Yes
Covariates x Call	No	No	No	No	Yes
N	965				

Note: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Based on two-tailed tests. Compliers only. Standard errors in parentheses. Covariates are turnout in seven previous elections, postal voter, gender, age, and electoral ward. Includes dummies for experimental blocks.

Figure 1: Variables and expected relationships

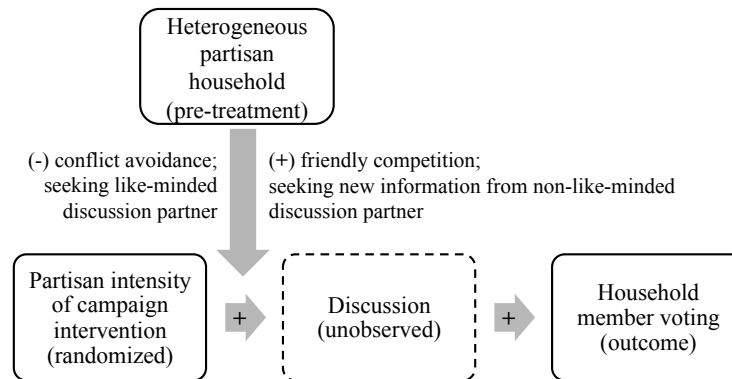
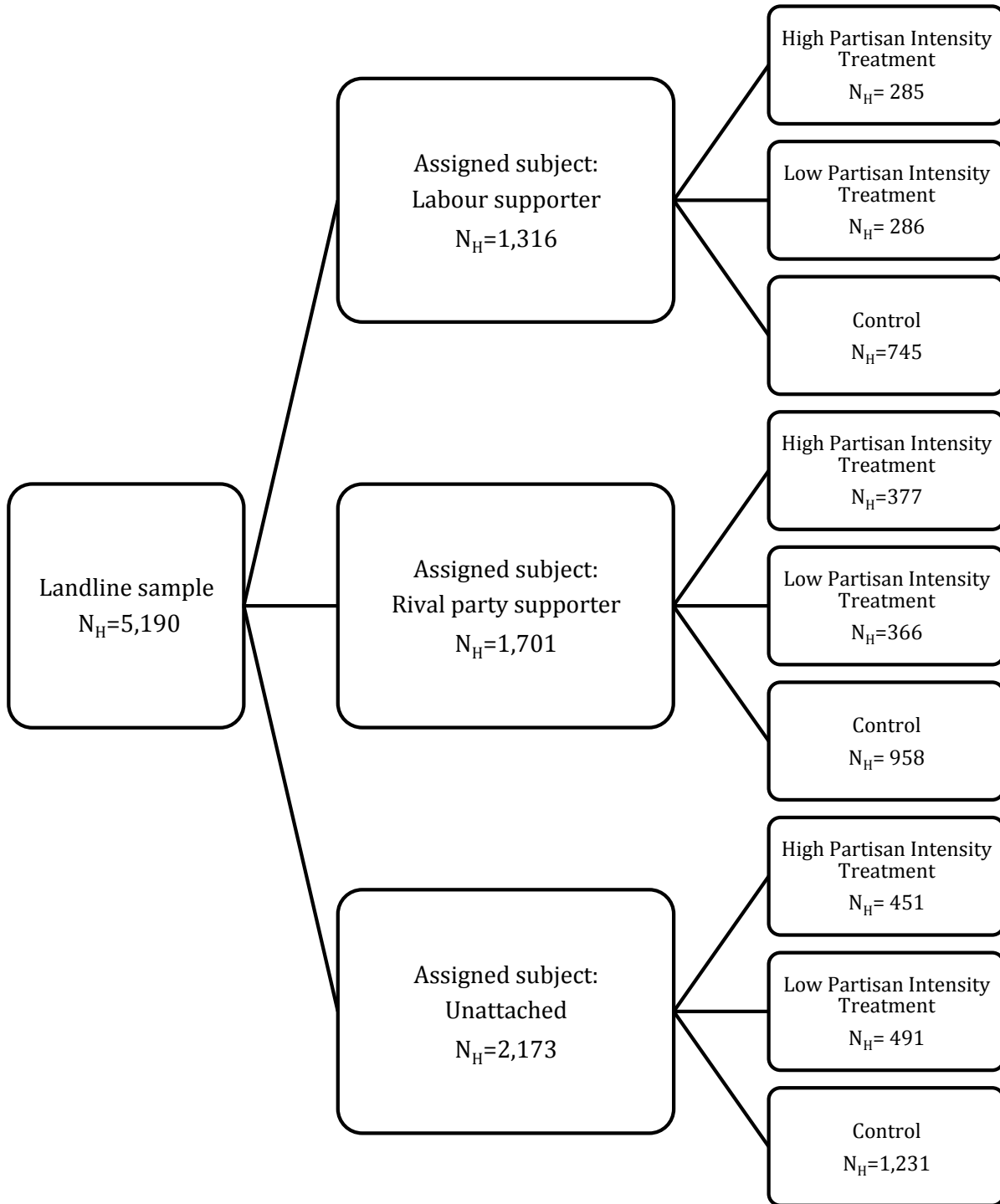
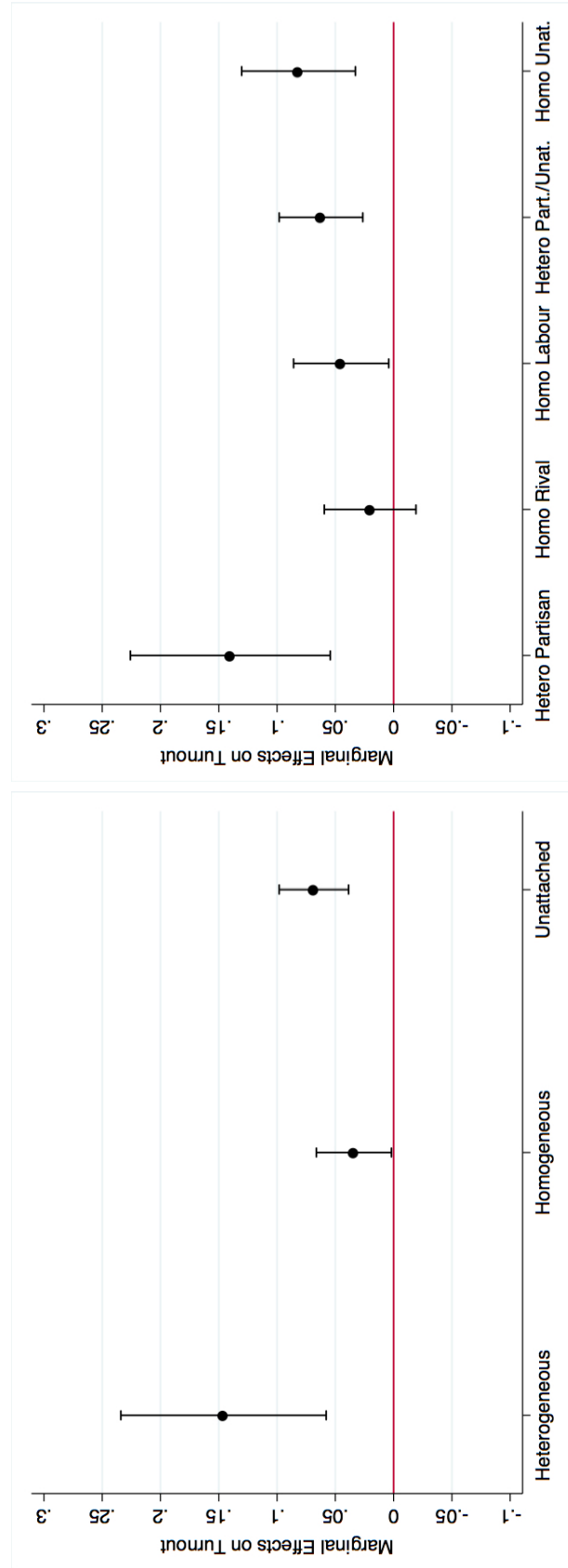


Figure 2: Experimental Assignment



N_H = Number of Households

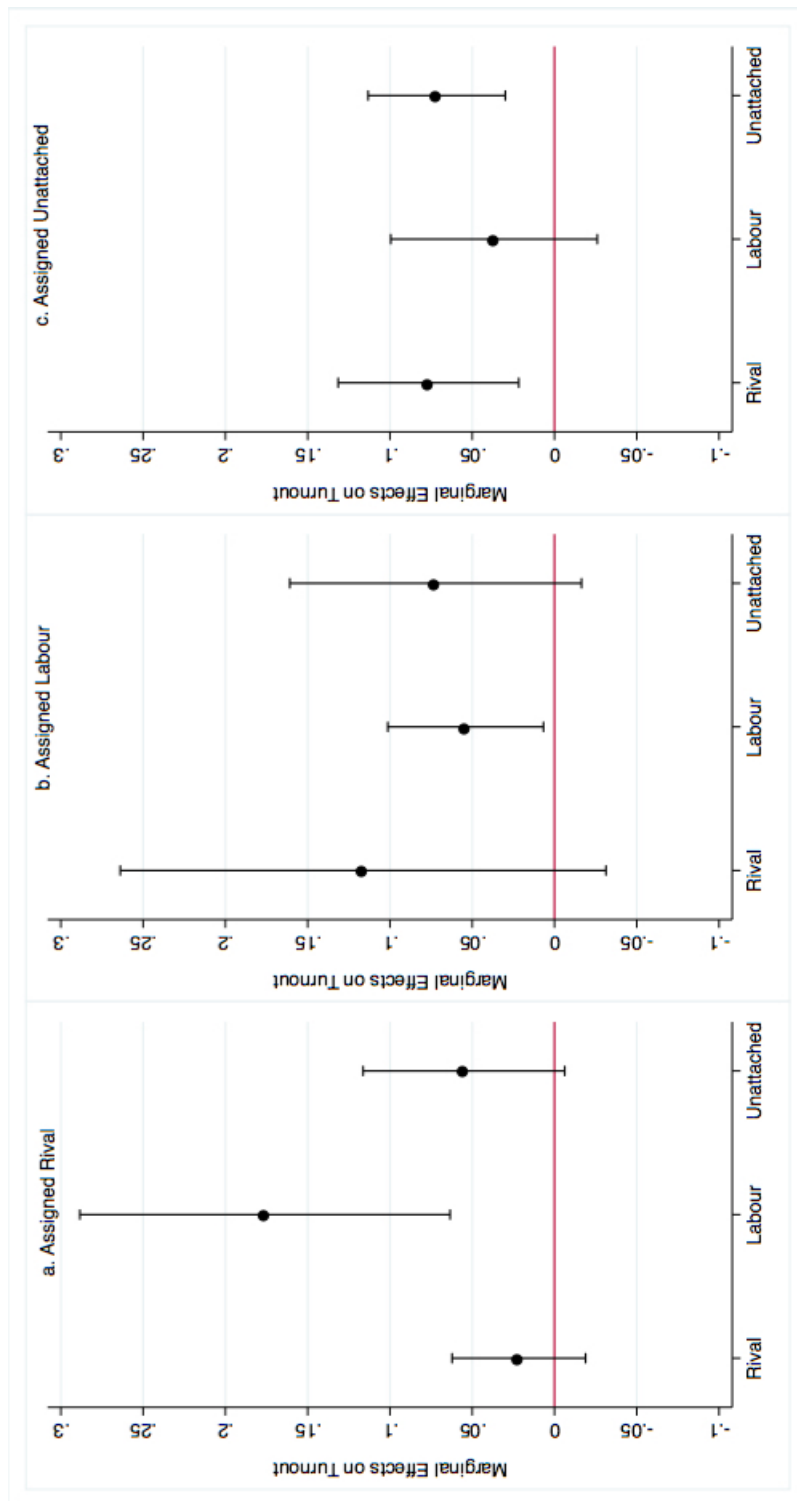
Figure 3: ITTs on turnout of unassigned household members conditional on household party preferences



a) Left: 3-group operationalization b) Right: 5-group operationalization

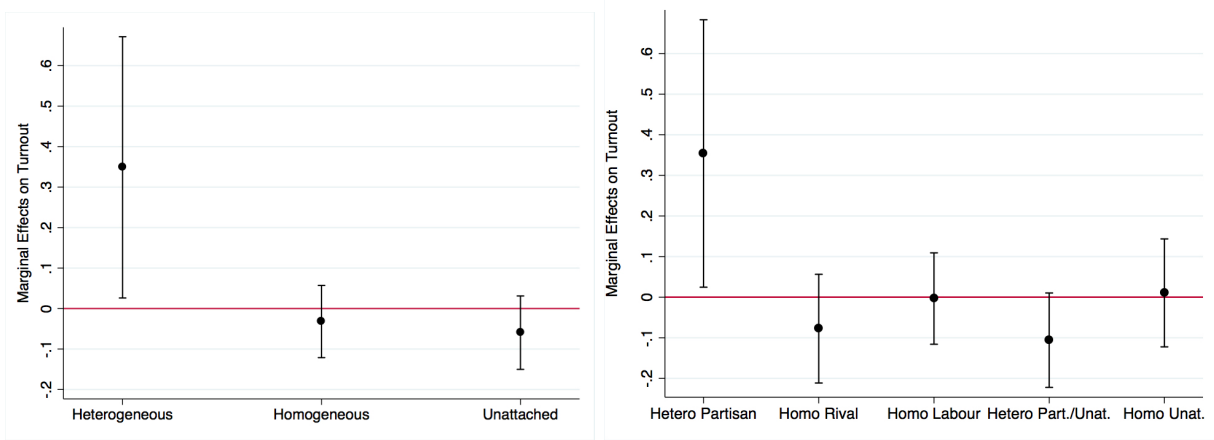
Note: 95% confidence intervals

Figure 4: Interactions between treatment assignment and party preferences of assigned and unassigned subject



Note: 95% confidence intervals

Figure 5: CACE high vs low partisan message conditional on household party preferences



a) Left: 3-group operationalization b) Right: 5-group operationalization

Note: 95% confidence intervals

Supporting Information

For Online Publication

All in the Family:

Partisan Disagreement and Electoral Mobilization in Intimate

Networks - a Spillover Experiment

Direct and indirect treatment effects

The effect of treatment on an individual can be defined as the difference between two potential outcomes: the outcome if an individual were treated and the outcome (at the same point in time) if the individual were not treated (Imbens and Rubin 2015; Rubin 1974, 1978; Gerber and Green 2012). By randomly assigning individuals to treatment and control groups we ensure that the potential outcomes of these groups of subjects are, in expectation, identical before applying the treatments. Under the three core-assumptions of independence, excludability and non-interference (SUTVA) (Imbens and Rubin 2015; Rubin 1986, 1980), the difference in the average outcomes of those subjects assigned to the treatment and those assigned to the control group will provide an unbiased estimator of the true Intent-to-Treat (ITT) effect (Gerber and Green 2012).¹

Let us say that in every two-voter household h one of two subjects, subject i_1 ($i_1 \in \{1, \dots, N_1\}$), was randomly sampled to be included in the experimental assignment, and randomly assigned to one of two treatment groups, or to the control group. The second subject in the household, subject i_2 ($i_2 \in \{1, \dots, N_2\}$), was randomly sampled to be excluded from experimental assignment. Let Z be the treatment indicator, indicating whether i_1 in household h was assigned to receive a low partisan intensity ($Z = 1$) or a high partisan intensity phone call ($Z = 2$) encouraging subjects to vote in the upcoming election, or to receive no call, i.e. the control group ($Z = 0$).

Our binary outcomes of interest, Y_{i_1} and Y_{i_2} are voter turnout. The potential outcomes for i_1 are defined as follows:

If $Z = 0$, then $Y_{i_1}(0) = 0$ or $Y_{i_1}(0) = 1$

If $Z = 1$, then $Y_{i_1}(1) = 0$ or $Y_{i_1}(1) = 1$

If $Z = 2$, then $Y_{i_1}(2) = 0$ or $Y_{i_1}(2) = 1$

Following the discussions in Gerber and Green (2012, chapter 2), and Imbens and Rubin (2015, chapter 1) the respective direct unit-level causal effects τ_{i_1} can then be defined as the

¹We estimate the ITT effect rather than the Average Treatment Effect (ATE) because not all subjects who were assigned to treatment in our experiment actually received the treatment.

difference between two potential outcomes:

$$\tau_{(10),i_1} = Y_{i_1}(1) - Y_{i_1}(0) \quad (1)$$

$$\tau_{(20),i_1} = Y_{i_1}(2) - Y_{i_1}(0) \quad (2)$$

$$\tau_{(21),i_1} = Y_{i_1}(2) - Y_{i_1}(1) \quad (3)$$

The direct ITT can be defined as the sum of each of the unit-level treatment assignment effects $\tau_{(10),i_1}$, $\tau_{(20),i_1}$, and $\tau_{(21),i_1}$ divided by N_1 (the total number of assigned subjects), and is equal to the difference in the average potential outcomes under the various treatment assignment conditions (Gerber and Green 2012). E.g. the ITT effect of assignment to the low partisan intensity treatment is:

$$\frac{1}{N_1} \sum_{i_1=1}^{N_1} (Y_{i_1}(1) - Y_{i_1}(0)) = \frac{1}{N_1} \sum_{i_1=1}^{N_1} Y_{i_1}(1) - \frac{1}{N_1} \sum_{i_1=1}^{N_1} Y_{i_1}(0) \quad (4)$$

Given random assignment to treatment and control conditions, excludability, and non-interference, the difference-in-means estimator is, in expectation, an unbiased estimator of the ITT. If we define m_1 as a subsample of N_1 consisting of subjects i_1 who were assigned to the low intensity partisan phone call and m_2 as a subsample of N_1 consisting of subjects i_1 who were assigned to the high intensity partisan phone call, then the direct ITT estimator of the low partisan intensity treatment can be defined as:

$$\widehat{ITT}_{(10),i_1} = \frac{1}{m_1} \sum_{i_1=1}^{m_1} Y_{i_1}(1) - \frac{1}{N_1 - (m_1 + m_2)} \sum_{i_1=(m_1+m_2+1)}^{N_1} Y_{i_1}(0) \quad (5)$$

Likewise for the high intensity partisan treatment, the direct ITT estimator can be identified as:

$$\widehat{ITT}_{(20),i_1} = \frac{1}{m_2} \sum_{i_1=(m_1+1)}^{m_2} Y_{i_1}(2) - \frac{1}{N_1 - (m_1 + m_2)} \sum_{i_1=(m_1+m_2+1)}^{N_1} Y_{i_1}(0) \quad (6)$$

And finally, the direct ITT estimator of the high versus the low intensity partisan treatment is:

$$\widehat{ITT}_{(21),i_1} = \frac{1}{m_2} \sum_{i_1=(m_1+1)}^{m_2} Y_{i_1}(2) - \frac{1}{m_1} \sum_{i_1=1}^{m_1} Y_{i_1}(1) \quad (7)$$

Similarly, the potential outcomes for i_2 are:

If $Z = 0$, then $Y_{i_2}(0) = 0$ or $Y_{i_2}(0) = 1$

If $Z = 1$, then $Y_{i_2}(1) = 0$ or $Y_{i_2}(1) = 1$

If $Z = 2$, then $Y_{i_2}(2) = 0$ or $Y_{i_2}(2) = 1$

And the respective indirect unit-level causal effects τ_{i_2} can then be defined as the difference between two potential outcomes:

$$\tau_{(10),i_2} = Y_{i_2}(1) - Y_{i_2}(0) \quad (8)$$

$$\tau_{(20),i_2} = Y_{i_2}(2) - Y_{i_2}(0) \quad (9)$$

$$\tau_{(21),i_2} = Y_{i_2}(2) - Y_{i_2}(1) \quad (10)$$

If we define m_3 as a subsample of N_2 consisting of household members i_2 of subjects i_1 who were assigned to the low intensity partisan phone call and m_4 as a subsample of N_2 consisting of household members i_2 of subjects i_1 who were assigned to the high intensity partisan phone call, then the indirect ITT estimator of assignment to the low partisan intensity treatment can be defined as:

$$\widehat{ITT}_{(10),i_2} = \frac{1}{m_3} \sum_{i_2=1}^{m_3} Y_{i_2}(1) - \frac{1}{N_2 - (m_3 + m_4)} \sum_{i_2=(m_3+m_4+1)}^{N_2} Y_{i_2}(0) \quad (11)$$

Likewise for the high intensity partisan treatment, the indirect ITT estimator can be identified as:

$$\widehat{ITT}_{(20),i_2} = \frac{1}{m_4} \sum_{i_2=(m_3+1)}^{m_4} Y_{i_2}(2) - \frac{1}{N_2 - (m_3 + m_4)} \sum_{i_2=(m_3+m_4+1)}^{N_2} Y_{i_2}(0) \quad (12)$$

And finally, the indirect ITT estimator of the high versus the low intensity partisan treatment is:

$$\widehat{ITT}_{(21),i_2} = \frac{1}{m_4} \sum_{i_2=(m_3+1)}^{m_4} Y_{i_2}(2) - \frac{1}{m_3} \sum_{i_2=1}^{m_3} Y_{i_2}(1) \quad (13)$$

In Table A3 (Model III) in the Supporting Information we report the results from a logistic regression model of turnout Y_{i_2} on assignment to either of the two treatments ($Z=1$ or $Z=2$), household partisan composition dummies, the interaction between treatment assignment and household partisan composition dummies, a n-by-k matrix of pre-treatment covariates (X) and the interaction between X and Z :

$$\begin{aligned} \text{Logit } Y_{i_2} = & \gamma_0 + \gamma_1 Z + \gamma_2 (\text{Labour})_{i_1} + \gamma_3 (\text{Unattached})_{i_1} + \gamma_4 (\text{Homogeneous})_h + \\ & \gamma_5 (\text{Unattached})_h + \gamma_6 Z (\text{Homogeneous})_h + \gamma_7 Z (\text{Unattached})_h + \\ & \Gamma X + \Gamma X Z + \epsilon_{i_2} \end{aligned} \quad (14)$$

In Table A5 (Model V) in the Supporting Information we report the estimation results from a logistic regression model of turnout Y_{i_2} on assignment to either of the two treatments Z ($Z=1$ or $Z=2$), the partisanship of the experimental subject, the partisanship of her household member and the two- and three-way interactions between the treatment and the party preferences of both household members. The model also includes a n-by-k matrix of

pre-treatment covariates (X) and the interaction between X and Z.

$$\begin{aligned}
\text{Logit } Y_{i_2} = & \gamma_0 + \gamma_1 Z + \gamma_2 (\text{Labour})_{i_1} + \gamma_3 (\text{Unattached})_{i_1} + \gamma_4 (\text{Labour})_{i_2} + \gamma_5 (\text{Unattached})_{i_2} + \\
& \gamma_6 (\text{Labour})_{i_1} (\text{Labour})_{i_2} + \gamma_7 (\text{Labour})_{i_1} (\text{Unattached})_{i_2} + \gamma_8 (\text{Unattached})_{i_1} (\text{Labour})_{i_2} + \\
& \gamma_9 (\text{Unattached})_{i_1} (\text{Unattached})_{i_2} + \gamma_{10} Z (\text{Labour})_{i_1} + \gamma_{11} Z (\text{Unattached})_{i_1} + \\
& \gamma_{12} Z (\text{Labour})_{i_2} + \gamma_{13} Z (\text{Unattached})_{i_2} + \gamma_{14} Z (\text{Labour})_{i_1} (\text{Labour})_{i_2} + \\
& \gamma_{15} Z (\text{Labour})_{i_1} (\text{Unattached})_{i_2} + \gamma_{16} Z (\text{Unattached})_{i_1} (\text{Labour})_{i_2} + \\
& \gamma_{17} Z (\text{Unattached})_{i_1} (\text{Unattached})_{i_2} + \Gamma X + \Gamma X Z + \epsilon_{i_2}
\end{aligned} \tag{15}$$

CACE estimators treatment versus control group

Table A2 in the Supporting Information reports the Complier Average Causal Effects (CACE), the average treatment effects for the subgroup of ‘compliers’ (Gerber and Green 2012, chapter 5). Compliers are defined as subjects i_1 and i_2 who live in households h that would be successfully contacted if the assigned member was assigned to receive either the high or the low partisan intensity phone call. We distinguish between Z , the treatment assignment indicator, and D , a contact indicator reporting whether i_1 in household h was successfully contacted with a low partisan intensity ($D = 1$, short D(1)) or a high partisan intensity phone call ($D = 2$, short D(2)), or was not contacted ($D = 0$, short D(0)).

Assuming one-sided non-compliance, the ITT_D effect of treatment assignment (Z) on receiving the treatment (D) equals the proportion of successfully contacted households (idem). For both of our treatment groups $ITT_d = .45$ (Table 1 in the manuscript).

The indirect CACE estimators of the two treatments compared to the control group for individuals i_1 can subsequently be defined as:

$$\widehat{CACE}_{(10),i_1} = \frac{\widehat{ITT}_{(10),i_1}}{\widehat{ITT}_{D(1)}} \tag{16}$$

and

$$\widehat{CACE}_{(20),i_1} = \frac{\widehat{ITT}_{(20),i_1}}{\widehat{ITT}_{D(2)}} \quad (17)$$

Similarly, the indirect CACE estimators for the two treatments compared to the control group for individuals i_2 can be defined as:

$$\widehat{CACE}_{(10),i_2} = \frac{\widehat{ITT}_{(10),i_2}}{\widehat{ITT}_{D(1)}} \quad (18)$$

and

$$\widehat{CACE}_{(20),i_2} = \frac{\widehat{ITT}_{(20),i_2}}{\widehat{ITT}_{D(2)}} \quad (19)$$

If Y_{i_2} was turnout for the unassigned household member, then the spillover CACE model we estimate can be formally written as:

$$Y_{i_2} = \beta_0 + \beta_1 D(1) + \beta_2 D(2) + \mu_{i_2}, \quad (20)$$

in which

$$D(1) = \gamma_0 + \gamma_1 Z(1) + \epsilon_{1,i_2} \quad (21)$$

and

$$D(2) = \delta_0 + \delta_1 Z(2) + \epsilon_{2,i_2} \quad (22)$$

CACE spillover estimator treatment versus treatment

Since (1) one subject per two-voter household, subject i_1 , was randomly sampled to be assigned to one of the three experimental groups, (2) the assigned subjects i_1 did not know in advance whether they were about to receive a high or the low partisan intensity call, and (3) compliance for unassigned household members i_2 is defined as living with a household member i_1 in household h who would answer the phone when called, it follows that the share of compliers among unassigned subjects in the high and the low partisan intensity call groups corresponds to the share of compliers among assigned household members. In expectation, the share of compliers in both treatment groups should be identical (Gerber, Green, Kaplan and Kern 2010, 302-305). Following Gerber et al. (2010) the latter "perfect blindness" assumption can be formalized as:

$$(D|Z = 2) = (D|Z = 1) \tag{23}$$

where Z indicates if subject i_1 in household h was assigned to treatment 2 (high partisan intensity message) or treatment 1 (low partisan intensity message), and D indicates whether subject i_1 in household h complied with $Z=2$ or $Z=1$.

If the perfect blindness assumption holds, then the indirect treatment effect $\tau_{(21),i_2}$ for the subgroup of unassigned compliers, defined as subjects i_2 living in household h with household member i_1 who would answer the phone if called with the high or the low intensity partisan phone call, can be identified as:

$$\tau_{(21),i_2} = E[Y_{i_2}(D = 2, Z = 2) - Y_{i_2}(D = 1, Z = 1)] \tag{24}$$

In Table 2 (Model V) of the manuscript we report the results from a logistic regression model of turnout Y_{i2} on compliance with the high partisan intensity treatment $D(2,1) = 2$ as opposed to compliance with the low partisan intensity treatment $D(2,1) = 1$, household partisan composition dummies, the interaction between $D(2,1)$ and household partisan composition dummies, pre-treatment covariates (X) and the interaction between X and $D(2,1)$:

$$\begin{aligned} \text{Logit } Y_{i2} = & \beta_0 + \beta_1 D(2,1) + \beta_2 (\text{Labour})_{i1} + \beta_3 (\text{Unattached})_{i1} + \beta_4 (\text{Homogeneous})_h + \\ & \beta_5 (\text{Unattached})_h + \beta_6 D(2,1) (\text{Homogeneous})_h + \beta_7 D(2,1) (\text{Unattached})_h + \\ & BX + BXD(2,1) + \mu_{i2} \end{aligned} \tag{25}$$

Randomization inference

In Table 1 in the manuscript, and Table A2 in the Supporting Information we report randomization-inference based p-values and confidence intervals.

Estimating p-values

Following Gerber and Green (2012) and Aronow and Samii (2012) we first calculate the differences-in-proportions between subjects assigned to treatment and subjects assigned to control. We then simulate a large number of hypothetical randomization outcomes under the assumption of the sharp null hypothesis that the Intent-to-Treat Effect (ITT) equals 0 for all subjects. If the ITT equaled 0 for all subjects, we could randomly reassign subjects to experimental groups and estimate the ITT resulting from every reassignment. If we re-assign subjects 10,000 times, we get the sampling distribution of ITTs under the assumption of no treatment effect for any subject. We then compare the differences-in-proportions estimate from our experimental data to the sampling distribution of all differences-in-proportions estimates over all hypothetical randomization outcomes.

Estimating confidence intervals

We need to further impose the constant treatment effect assumption at the ITT level to estimate confidence intervals using randomization-inference (Gerber and Green 2012, 67). Following Gerber and Green (2012, 67-68) we impute unobserved treated potential outcomes by adding the ITT we estimated from our experiment to the observed outcomes in the control group. We then impute unobserved untreated potential outcomes by subtracting our estimated ITT from the observed outcomes in the treatment group. We then use this full schedule of potential outcomes to re-assign subjects to treatment and control group 10,000 times. Finally, we list the estimated ITTs from each random re-assignment in ascending order: "The 2.5th percentile marks the bottom of the 95% confidence interval, and the estimate at the 97.5th percentile marks the top" (Gerber and Green 2012, 67). The interpretation of the 95% confidence interval is the following: If we imagine a series of hypothetical random assignments, 9,500 out of 10,000 random assignments will generate intervals that bracket the true (population) ITT.

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Table A1: Covariate balance among non-experimental subjects

	Combined Sample			Heterogeneous Households			Homogeneous Households			Unattached Households		
	Control	Low	High	Control	Low	High	Control	Low	High	Control	Low	High
Mean Age	36.9	37.5	36.4	38.6	39.4	40.1	38.8	39.4	37.9	35.5	36.1	35.0
Age Missing	60.7	60.7	61.4	55.0	64.1	47.3	57.5	55.9	63.3	63.4	63.8	61.4
Female	46.3	45.7	44.9	40.0	48.7	40.0	47.0	46.0	46.8	46.3	45.2	44.1
Gender Unknown	10.1	09.3	09.7	13.3	02.6	05.5	09.5	10.0	08.2	10.2	09.3	11.1
Postal	15.8	14.0	15.8	23.3	10.3	20.0	18.2	17.5	20.4	13.6	11.8	12.2
Ward 1	25.9	29.3	25.3	24.2	20.5	21.8	19.1	23.0	22.1	30.6	34.1	28.0
Ward 2	13.7	14.5	13.7	12.5	20.5	20.0	17.7	18.7	17.5	11.1	11.3	10.5
Ward 3	24.7	24.5	26.7	27.5	25.6	30.9	28.2	27.7	27.8	22.0	22.2	25.6
Ward 4	35.7	31.7	34.2	35.8	33.3	27.3	34.9	30.6	32.6	36.3	32.4	36.0
Voted Local 2012	54.7	55.0	56.6	60.0	61.5	67.3	69.8	69.7	70.7	43.8	44.6	45.5
Voted Local 2011	62.1	62.3	61.8	66.7	64.1	67.3	77.3	77.0	76.0	51.3	52.2	51.1
Voted General 2010	77.2	78.5	78.0	81.7	76.9	83.6	88.9	87.4	90.6	68.8	72.5	68.4
Voted Election 1	45.6	44.0	43.7	50.0	48.7	41.8	57.4	55.9	55.9	37.1	35.6	35.2
Voted Election 2	43.1	41.1	43.0	47.5	43.6	50.9	55.9	54.5	55.9	33.9	31.9	33.1
Voted Election 3	50.6	49.6	52.1	61.7	53.8	61.8	63.1	62.1	65.2	41.1	40.9	41.9
Voted Election 4	42.9	43.6	43.2	44.2	51.3	49.1	53.9	55.2	52.0	35.1	35.3	36.4
N	2793	1082	1055	120	39	55	1093	422	417	1580	621	583

Note: Figures exclude households with missing turnout data.

Table A2: Direct and indirect mobilization effects, covariate-adjusted

	Labour	Rival Party	Unattached	Combined
	Intent-to-Treat Effect			
	Direct Effects on Assigned Subjects			
Low Partisan Intensity Call	1.8	2.4	6.0***	3.8**
	[-3.8, 7.3]	[-2.2, 7.2]	[2.8, 9.2]	[1.3, 6.4]
N	996	1272	1607	3875
High Partisan Intensity Call	5.9*	1.1	3.4*	3.3**
	[0.4, 11.4]	[-3.4, 5.9]	[0.3, 6.7]	[0.8, 5.7]
N	997	1285	1566	3848
	Indirect Effects on Unassigned Subjects			
Low Partisan Intensity Call	4.5	4.6	6.6***	5.4***
	[-0.8, 9.7]	[-0.1, 9.4]	[3.3, 9.9]	[2.9, 7.9]
N	996	1272	1607	3875
High Partisan Intensity Call	5.5*	3.2	3.7*	3.9**
	[0.2, 10.8]	[-1.5, 7.8]	[0.4, 7.1]	[1.4, 6.3]
N	997	1285	1566	3848
	Complier Average Causal Effect			
	Direct Effects on Assigned Subjects			
Low Partisan Intensity Contact	3.2	5.5	14.6**	8.5**
	(5.5)	(5.3)	(4.7)	(3.0)
N	996	1272	1607	3875
High Partisan Intensity Contact	12.5*	2.2	8.4	7.3*
	(5.9)	(4.8)	(4.7)	(2.9)
N	997	1285	1566	3848
	Indirect Effects on Unassigned Subjects			
Low Partisan Intensity Contact	8.6	9.8	16.7***	11.9***
	(5.4)	(5.4)	(4.7)	(2.9)
N	996	1272	1607	3875
High Partisan Intensity Contact	11.6*	6.6	9.6*	8.6**
	(5.9)	(4.8)	(4.8)	(2.9)
N	997	1285	1566	3848

Note: *** p<0.001, ** p<0.01, * p<0.05, based on two-tailed tests, standard errors in parentheses, randomization inference-based 95%-Confidence Intervals in brackets. Covariates are turnout in seven previous elections, postal voter, gender, age and electoral ward.

Table A3: Logistic and IV regression of turnout of unassigned subject on treatment assignment of assigned subject, conditional on household party preferences

	I	II	III
	Logistic Regression Results (ITT)		
Telephone Call	.764* (.311)	1.103** (.360)	1.582* (.643)
Heterogeneous		Reference	
Homogeneous	.375 (.235)	.270 (.267)	.277 (.279)
Unattached	-.237 (.248)	.141 (.284)	.154 (.296)
Heterogeneous x Call		Reference	
Homogeneous x Call	-.657* (.327)	-.945* (.378)	-.917* (.378)
Unattached x Call	-.412 (.328)	-.592 (.379)	-.606 (.380)
	Instrumental Variable Linear Regression Results (CACE)		
Contact with canvasser	.320* (.134)	.357** (.120)	.332* (.168)
Heterogeneous		Reference	
Homogeneous	.069 (.040)	.051 (.037)	.052 (.037)
Unattached	-.066 (.038)	.023 (.036)	.019 (.036)
Heterogeneous x contact		Reference	
Homogeneous x contact	-.278* (.140)	-.312* (.125)	-.323* (.128)
Unattached x contact	-.200 (.139)	-.223 (.124)	-.210 (.126)
Covariates	No	Yes	Yes
Covariates x Treatment	No	No	Yes
Observations	4930		

Note: *** p<0.001, ** p<0.01, * p<0.05, based on two-tailed tests, standard errors in parentheses. Covariates are turnout in seven previous elections, postal voter, gender, age and electoral ward. Includes dummies for experimental blocks.

Table A4: Robustness Check: Logistic regression of turnout of unassigned subject on treatment assignment of assigned subject, conditional on household party preferences, 5-categories operationalization

	I	II	III
	Logistic Regression Results (ITT)		
Telephone Call	.763*	1.104**	1.548*
	(.312)	(.362)	(.651)
Heterogeneous		Reference	
Homogeneous Rival	.385	.318	.333
	(.258)	(.297)	(.302)
Homogeneous Labour	.361	.207	.212
	(.257)	(.292)	(.296)
Partisan x Unattached	-.218	.158	.169
	(.252)	(.290)	(.294)
Homogeneous Unattached	-.641*	.310	.317
	(.286)	(.330)	(.334)
Heterogeneous x Call		Reference	
Homogeneous Rival x Call	-.693*	-1.067**	-1.033**
	(.341)	(.395)	(.397)
Homogeneous Labour x Call	-.611	-.801*	-.791*
	(.346)	(.400)	(.402)
Partisan x Unattached x Call	-.453	-.630	-.636
	(.339)	(.394)	(.395)
Homogeneous Unattached x Call	-.371	-.521	-.551
	(.353)	(.410)	(.412)
Covariates	No	Yes	Yes
Covariates x Treatment	No	No	Yes
Observations	4930		

Note: *** p<0.001, ** p<0.01, * p<0.05, based on two-tailed tests, standard errors in parentheses. Covariates are turnout in seven previous elections, postal voter, gender, age, and electoral ward. Includes dummies for experimental blocks.

Table A5: Logistic regression of turnout on treatment assignment and party preference of both subjects, and interactions between assignment and party preferences

	I	II	III
Telephone Call	.070 (.137)	.038 (.157)	.538 (.580)
Assigned Rival	Reference		
Assigned Lab	-.184 (.302)	-.031 (.347)	-.030 (.353)
Assigned Unattached	-.659** (.196)	-.457* (.225)	-.451* (.228)
Unassigned Rival	Reference		
Unassigned Lab	-.696 (.376)	-.470 (.421)	-.487 (.427)
Unassigned Unattached	-.527** (.180)	-.128 (.210)	-.131 (.214)
Assigned Rival x Unassigned Rival	Reference		
Assigned Lab x Unassigned Lab	.829 (.484)	.572 (.548)	.591 (.556)
Assigned Lab x Unassigned Unattached	.067 (.400)	.188 (.465)	.219 (.474)
Assigned Unattached x Unassigned Lab	.422 (.476)	.536 (.532)	.576 (.538)
Assigned Unattached x Unassigned Unattached	.053 (.275)	.331 (.318)	.346 (.324)
Assigned Rival x Call	Reference		
Assigned Lab x Call	.228 (.482)	.730 (.557)	.685 (.559)
Assigned Unattached x Call	.440 (.277)	.634* (.323)	.615 (.324)
Unassigned Rival x Call	Reference		
Unassigned Lab x Call	1.141* (.473)	1.354* (.541)	1.323* (.544)
Unassigned Unattached x Call	.092 (.271)	.348 (.315)	.308 (.317)
Assigned Rival x Unassigned Rival x Call	Reference		
Assigned Lab x Unassigned Lab x Cal	-1.288 (.678)	-1.820* (.780)	-1.770* (.783)
Assigned Lab x Unassigned Unattached x Call	-.083 (.612)	-.632 (.715)	-.624 (.716)
Assigned Unattached x Unassigned Lab x Call	-1.376* (.622)	-1.722* (.709)	-1.720* (.710)
Assigned Unattached x Unassigned Unattached x Call	-.210 (.398)	-.437 (.465)	-.447 (.467)
Covariates	No	Yes	Yes
Covariates x Treatment	No	No	Yes
Observations	4930		

Note: *** p<0.001, ** p<0.01, * p<0.05, based on two-tailed tests, standard errors in parentheses. Covariates are turnout in seven previous elections, postal voter, gender, age and electoral ward. Includes dummies for experimental blocks.

Table A6: Robustness Check: Logistic Regression Results: CACE of high vs low partisan intensity call on turnout of unassigned subjects, conditional on household party preferences, 5-categories operationalization

	I	II	III	IV	V
Partisan Phone Contact	-.098 (.142)	-.154 (.165)	1.347 (.689)	1.403 (.785)	1.780 (1.370)
Heterogeneous Partisan	Reference Group				
Homogeneous Rival	-.130 (.369)	-.559 (.432)	.776 (.622)	.467 (.686)	.593 (.693)
Homogeneous Labour	-.291 (.392)	-.844 (.466)	.595 (.623)	.018 (.708)	.231 (.720)
Partisan x Unattached	-.567 (.365)	-.636 (.431)	.455 (.609)	.451 (.684)	.649 (.695)
Homogeneous Unattached	-1.230** (.442)	-.829 (.520)	-.360 (.670)	.035 (.755)	.161 (.765)
Heterogeneous x Partisan Contact	Reference Group				
Homogeneous Rival x Partisan Contact			-1.461 (.746)	-1.753* (.850)	-2.065* (.877)
Homogeneous Labour x Partisan Contact			-1.440 (.746)	-1.414 (.850)	-1.653 (.867)
Partisan x Unattached x Partisan Contact			-1.721* (.739)	-1.890* (.847)	-2.232* (.869)
Homogeneous Unattached x Partisan Contact			-1.371 (.773)	-1.381 (.884)	-1.567 (.901)
Covariates	No	Yes	No	Yes	Yes
Covariates x Call	No	No	No	No	Yes
N	965				

Note: *** p<0.001, ** p<0.01, * p<0.05, based on two-tailed tests, standard errors in parentheses. Covariates are turnout in seven previous elections, postal voter, gender, age, and electoral ward. Includes dummies for experimental blocks.

Table A7: Comparison between official City Council elections results for May 2011 and May 2012, and complete November 2012 Labour Party database^a (pre-treatment) across all electoral wards included in the experiment

	2011 Elections		2012 Elections	
	Official Results	Labour Database	Official Results	Labour Database
	Party support among voters (%)			
Labour Party	43.5	36.0	45.9	38.7
Rival Party	56.5	31.9	54.1	31.2
Unattached	-	32.1	-	30.1
	Coverage of Database			
N registered voters	70,443 ^b	26,827	- ^c	26,827
% in database		38.1		38.1 ^d
N of votes	27,626	14,579	22,371	12,921
Turnout (%)	39.2	54.5	- ^c	48.3

Note: Each electoral ward is represented by three city councillors, who serve four year terms. One councillor is elected every year. In the fourth year there are no elections.

^a The complete database includes registered voters with available landline and mobile phone numbers regardless of household size.

^b The number of registered voters is estimated using the number of votes and turnout in the 2011 elections.

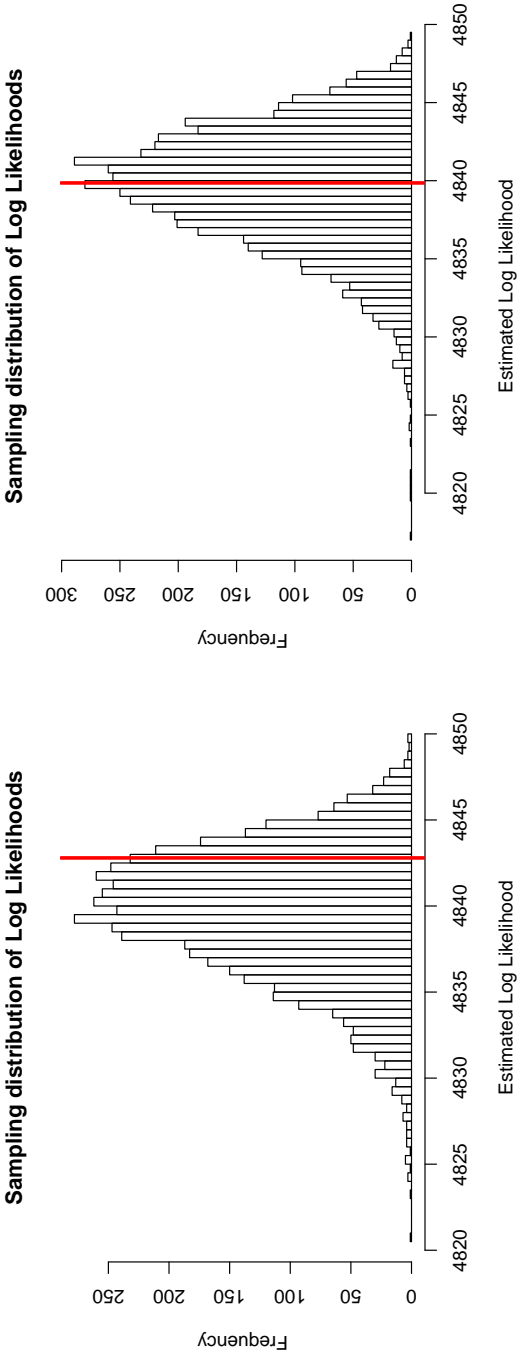
^c Turnout data for May 2012 City Council elections is not available on the Birmingham City Council website.

^d The percentage of all registered voters included in the database is estimated using the number of registered voters in the database and the (estimated) total number of registered voters for 2011.

Source: Birmingham City Council. Birmingham Elections 2011 and Election Results - City Council Elections 3 May 2012. Accessed 10 September 2015: <http://www.birmingham.gov.uk/election>

Figure A1: Balance Check: Estimated log likelihood resulting from multinomial logistic regression of treatment assignment of household member on pre-treatment covariates compared to sampling distributions of simulated log-likelihood statistics under sharp null

a) Left: Assigned subjects ($p=.20$) b) Right: Household members ($p=.50$)



Randomization inference

Following Gerber and Green (2012) and Aronow and Samii (2012) we first calculate the log likelihood from a multinomial logistic regression of treatment assignment on pre-treatment covariates. We then simulate a large number of hypothetical randomization outcomes under the assumption that the log likelihood equals zero for all subjects. Under the assumption that the sharp null hypothesis is true and the log likelihood is 0 for all subjects, we can randomly reassign subjects to treatments and control and estimate the log likelihood resulting from every reassignment. If we reassign subjects 10,000 times, we get the sampling distribution of all log likelihoods under the assumption that the covariates taken together do not predict treatment assignment for any subject. Finally we compare the log likelihood estimate from our experimental data to the distribution of all log likelihood estimates over all hypothetical randomization outcomes.

Figure A2: Attrition Check: Estimated log likelihood resulting from multinomial logistic regression of treatment assignment on missing outcome data compared to sampling distributions of simulated log-likelihood statistics under sharp null

a) Left: Excluding household composition interactions ($p=.26$) b) Right: Including household composition interactions ($p=.32$)

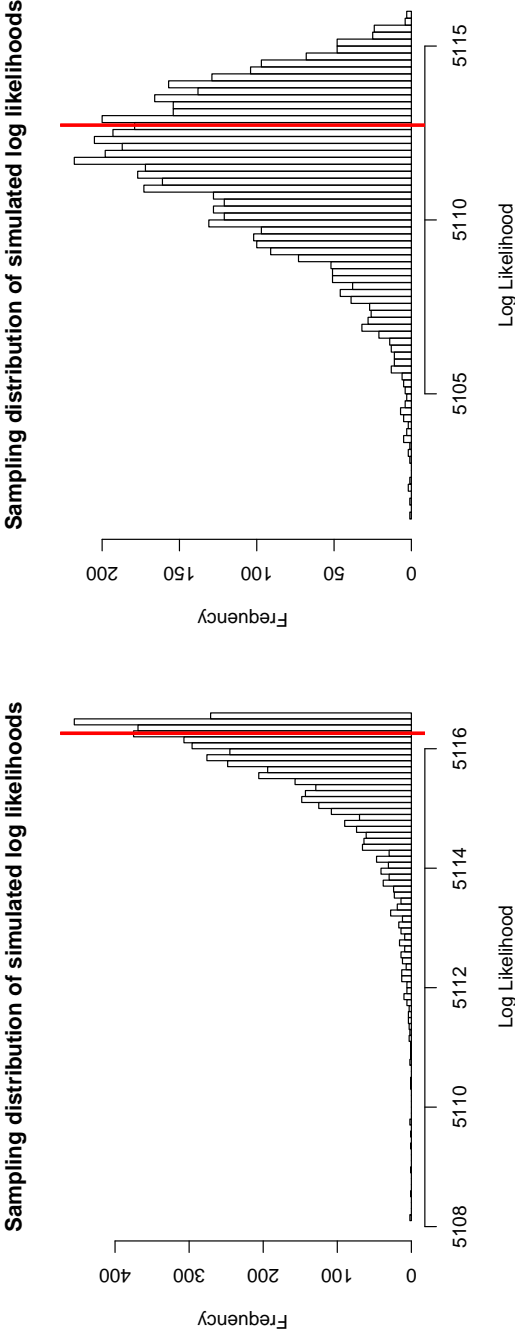


Figure A3: High Partisan Intensity Script

2012 PCC Elections Calling Script

When you make a call tick a box in the “Call Attempted” row. Do not leave an answer phone message unless it is the fifth call and we have not made contact yet. Do not call again if a contact has been made (i.e. the larger Question boxes have been filled in), similarly don’t write anything in these boxes unless you make a contact or establish that it is a wrong number.

We have a message that we would like you to deliver. You can do it in a conversational manner but please do try and hit all the talking points in the message.

Please do make sure to mention that [candidate name] is the Labour party candidate. This is to preserve the integrity of this experiment which will greatly help us in the long run.

Message:

“Hello, my name is I am phoning from your local Labour Party. I just wanted to remind you to go out and vote for Labour candidate [candidate name] in the Police and Crime Commissioner Election on Thursday. Your local polling station is located at ... during the usual opening hours from 7am to 10pm. Have you heard of the Labour candidate [candidate name]?”

Labour’s [candidate name] is determined to fight the Tory cuts to frontline policing that will hit Birmingham hard if a Conservative is elected. The Conservatives have sacked Police Officers and closed down Police Stations. In contrast, the Labour Party put more Police Officers on the ground and will protect police numbers.

- Are you going to vote for a Police Commissioner?
- Which candidate/party are you going to support in this election?
- If there was a General Election tomorrow, which party would you support?

Thanks a lot for taking the time to talk to me.

Voice message: On the 5th attempt leave a voice message with the above content but without the ending questions.ⁱ

Fill in the boxes 1 to 5 according to the criteria laid out on the next page:

Q1	Q2	Q3	Q4	Q5
----	----	----	----	----

ⁱ Due to the limited number of volunteers there was a maximum of three attempts made at reaching subjects. Two hours before polls closed on Election Day, some volunteers started to leave a small number of personalised messages (addressed to the assigned subject) on mailboxes amounting to 16% of the treatment sample. Due to the fact that these messages were delivered very late on Election Day to phones that had not been answered after two attempts, we judge the probability that they significantly violated the exclusion restriction to be very low.

Figure A4: Low Partisan Intensity Script

2012 PCC Elections Calling Script

When you make a call tick a box in the “Call Attempted” row. Do not leave an answer phone message unless it is the fifth call and we have not made contact yet. Do not call again if a contact has been made (i.e. the larger Question boxes have been filled in), similarly don’t write anything in these boxes unless you make a contact or establish that it is a wrong number.

We have a message that we would like you to deliver. You can do it in a conversational manner but please do try and hit all the talking points in the message.

Please do NOT mention that [candidate name] is the Labour party candidate unless the contact brings it up or asks you which party he represents. This is to preserve the integrity of this experiment which will greatly help us in the long run.

Message:

“Hello, my name is I am phoning to remind you to go out and vote for [candidate name] in the Police and Crime Commissioner Election on Thursday. Your local polling station is located at ... during the usual opening hours from 7am to 10pm. Have you heard of [candidate name]?

[Candidate name] is a candidate for Police and Crime Commissioner and he is determined to fight the cuts in frontline policing. As [former role] [candidate’s first name] has a strong record in reducing crime and protecting our Police Force. [Candidate name] has been fighting for the victims of crime for over 30 years.

- Are you going to vote for a Police Commissioner?
- Which candidate/party are you going to support in this election?

Thanks a lot for taking the time to talk to me.

Voice message: On the 5th attempt leave a voice message with the above content but without the ending questions.

Fill in the boxes 1 to 5 according to the criteria laid out on the next page:

Q1	Q2	Q3	Q4	Q5
----	----	----	----	----

Figure A5: Questionnaire

Filling in Q1-5:

Q1

tells us the status of the call so that we can analyse if and how contact was made. Use the following codes to indicate this status:

1. Conversation with the specific individual i.e. you spoke to them and they didn't ask you to "call back later"
2. Voice message left; do not leave a message unless it is the fifth attempt to contact
3. Wrong number i.e. number is for a different address/family or the specific individual has moved away
4. Number not recognised i.e. line is dead or is a fax/modem line

Q2

tells us whether the message was delivered in full; please use the following codes:

1. Full message delivered
2. Individual ends the conversation before you can deliver the full message and does not ask you to "call back later"; if you are asked to call back later leave all of the question boxes blank and we will call through the list again later
3. Individual has already voted i.e. postal voter

Q3

tells us if the individual is interested in which party [candidate name] represents

1. Individual asks you which party [candidate name] represents
2. Individual knows and mentions that [candidate name] represents Labour

Q4

tells us how the person will vote in the PCC election. Please use the following codes:

L	Labour	B	UKIP / [candidate name]
A	Against Labour	I	Independent / [candidate names]
D	Don't Know	Z	Not voting in PCC elections
X	Won't say	J	Will vote for [candidate name] specifically (rather than just the Labour candidate)
T	Conservative / [candidate name]	O	Will vote against [candidate name] personally (rather than just generally Against Labour [A])
S	Lib Dem / [candidate name]		

Q5

IF the individual mentions that [candidate name] is Labour or asks what party he represents, please finish by asking which party they would support if there was a General Election tomorrow and use the following codes:

L	Labour	A	Against i.e. not Labour
T	Conservative	D	Don't Know
S	Lib Dem	X	Won't Say
G	Green	V	BNP
B	UKIP	Z	Won't vote